dent has sent to me an unmistakable specimen of annelid piping from a rock at Killin. Now that we are all on the scent, I have no doubt that my Loch Fyne worms will be found at the head of a long procession. ARGYLL.

Inveraray, January 28.

Mr. Howorth on the Variation of Colour in Birds.

PERMIT me to point out that the fact cited by Mr. Howorth (*antea*, p. 294), of the similarity between the birds of Western Europe and those of Japan, which, according to him, illustrates "from an unexpected quarter" the views expressed in his work "The Mammoth and the Flood," is by no means so novel as the general reader might, from his communication, suppose. On the contrary, it has been before the world for more than half a century, and is, or ought to be, familiar to every well-informed ornithologist, since it was in 1835 that Temminck gave ("Manuel d'Ornithologie," Part 3, Introd. pp. l.-liii.) a list of 114 species of birds, which he said were common to the Japanese Empire and to Europe. It has subsequently been shown that this number was exaggerated; but, as observed in 1857 by Mr. Sclater, in his classical paper on the geographical distribution of birds, "there can be no question as to the general strong resemblance of the Japanese avifauna to that of Europe" (Journ. Linn. Soc., Zoology, ii. p. 134).

That the birds of Siberia differ in appearance from those of Japan and of Western Europe has also been abundantly shown by the long succession of illustrious explorers and naturalists from whom we derive all but an infinitesimally small portion of our knowledge of Siberian and Central Asiatic zoology; but Mr. Howorth does not seem to be aware that another hypothesis has usually been brought forward to account for that difference, as well as for the similarity of the Japanese avifauna and our own. In regard to the latter, this hypothesis has been propounded to my certain knowledge for more than five-and-twenty years (*Ibis*, 1863, p. 189); and, whether true or false, has not, so far as I am aware, been refuted. It is that the resemblance is an effect due to "the prevalence in both localities of an insular, as opposed to a continental climate," for it has been observed over and over again in various parts of the world (and not only in birds) that island forms possess a darker coloration than their continental representatives (*cf.* Gould, Proceedings of the Zoological Society, 1855, p. 78), while the fact that neither British nor Japanese birds exhibit the brilliant hues of their continental kinsfolk is notorious.

Another remark upon Mr. Howorth's communication I would offer. His theory, of which he has "little doubt," that "the willow grouse of the Continent is an altered form, and that our red grouse is the parent," is not new, but was definitely stated some eight or nine years ago in the "Encyclopædia Britannica," from the article "Grouse" in which I venture to quote a passage showing that there is a side of the question which he does not seem to have considered.

"A very interesting subject for discussion would be whether *Lagopus scoticus* or *L. albus* has varied most from the common stock of both. We can here but briefly indicate the more salient points that might arise. Looking to the fact that the former is the only species of the genus which does not assume white clothing in winter, an evolutionist might at first deem the variation greatest in its case; but then it must be borne in mind that the species of *Lagopus* which turn white differ in that respect from all other groups of the family *Tetraonidæ*. Furthermore, it must be remembered that every species of *Lagopus* (even *L. leucurus*, the whitest of all) has its first set of *remiges* coloured brown. These are dropped when the bird is about half-grown, and in all the species but *L. scoticus* white *remiges* are then produced. If, therefore, as is generally held, the successive phases assumed by any animal in the course of its progress to maturity indicate the phases through which the species has passed, there may have been a time when all the species of *Lagopus* wore a brown livery even when adult, and the white dress donned in winter has been imposed upon the wearers by causes that can be easily suggested, for it has been freely admitted by naturalists of all schools that the white plumage of the birds of this group protects them from danger during the snows of a protracted winter. On the other hand, it is not at all inconceivable that the Red Grouse, instead of perpetuating directly the more ancient properties of an original *Lagopus* that underwent no great seasonal change of plumage, may derive its ancestry from the widely-ranging Willow-Grouse, which, in an epoch comparatively recent (in the geological sense), may have stocked Britain, and left descendants that, under conditions in which the assumption of a white garb would be almost fatal to the preservation of the species, have reverted (though doubtless with some modifications) to a comparative immutability essentially the same as that of the primal *Lagopus*."

In conclusion, let me recommend those interested in the local variation of colouring in the plumage of birds to study Gloger's "Das Abändern der Vögel durch Einfluss des Klimas" (Breslau, 1833), a treatise which, though naturally out of date in many respects, contains much that ornithologists would be the better for not overlooking. ALFRED NEWTON.

Magdalene College, Cambridge, January 26.

Constitution of the Chlorides of Aluminium and the Allied Metals.

IN NATURE of December 27 (pp. 198-200), Dr. Young has directed the attention of your readers to the formulæ of the so-called hexachlorides, R_2Cl_6, and bodies of similar constitution. As this question possesses a great theoretical interest, I may be allowed to say a few words about it.

The constitution of the chlorides R_2Cl_6 was regarded by Friedel as analogous to that of carbon hexachloride, $Cl_3\equiv C-C\equiv Cl_3$, and the metals themselves were said to be tetravalent. Mendelejeff, in his classical paper on the periodic law—a gem of chemical literature—was the first to point out that it is not necessary to regard aluminium in Al_2Cl_6 as tetravalent, but that a compound of this formula represents only a molecular combination—a polymeric state—of $AlCl_3$. This view was very little regarded by chemists. It was, ten years later (Proc. Vienna Acad., 1882), extended by the author of the present lines, to other bodies of similar constitution, and it was shown that several chlorides give a vapour consisting partly or entirely of polymeric modifications of the simple chloride. So we have the molecules $Sn_2Cl_4-SnCl_2$, $Fe_2Cl_4-FeCl_2$ ($N_2O_4-NO_2$), and it was supposed that other substances of similar constitution would split up into simpler molecules, if they could bear higher temperatures, especially Al_2Cl_6, aluminium belonging to a natural group of *triad* elements, whose other members, viz. B and In, give chlorides of the simple and molecular formulæ, BCl_3 and $InCl_3$. As further instances of the above case may be quoted to-day, H_2F_2-HF, $Be_2Cl_4-BeCl_2$, $Ga_2Cl_6-GaCl_3$.

It may, however, be useful to those readers of NATURE who cannot follow completely the current chemical literature, to know that the values of vapour-densities obtained by Dumas's method are not strictly comparable with those obtained by V. Meyer's method; for, as has been shown lately by Victor Meyer and his pupils, especially Biltz, the vapour density of dissociable substances is found smaller by V. Meyer's apparatus than by that of Dumas at one and the same temperature. So Dumas's method shows, for the above chlorides, the existence of double molecules at a temperature at which that of V. Meyer indicates single molecules. It was already pointed out by Dr. Young that the single molecules, $GaCl_2$, $InCl_2$, $InCl_3$, and $CrCl_3$, were found by V. Meyer's method only, and there is no doubt that some of them may be found double by Dumas's method.

The result of the vapour-density determinations of aluminium chloride and the chlorides of the allied metals, carefully collected by Dr. Young in NATURE, is of great theoretical importance, for, as the chlorides RCl_3 exist as single molecules in the gaseous state, the respective metals are undoubtedly *trivalent* in these compounds.

But the question arises, what is the valency of these metals when their chlorides have the double molecule R_2Cl_6?

It is impossible to regard these compounds as analogous to carbon-hexachloride and the said metals as tetravalent, for that compound, when heated to a higher temperature, does *not* split up into two molecules, CCl_3, as the chlorides R_2Cl_6 do. Moreover, it must be admitted that the theory of valency which was developed by the study of organic compounds (*carbon* derivatives) does not strictly hold good in the case of the remaining elements. It is impossible to state the number of "bonds" by which the chlorides RCl_3 are kept together in double molecules, R_2Cl_6. This number is certainly not *one* (Cl_3R-RCl_3); just as two molecules of $SnCl_2$ in Sn_2Cl_4 cannot be regarded as being united by one bond, $Cl_2Sn'''-Sn'''Cl_2$, for no one will assume that tin is trivalent in this compound. If we admit any bonds at all between the two Sn atoms, we must admit two, $Cl_2Sn=$

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$SnCl_2$. Again, silica (as has been shown by Mendelejeff long before Henry) certainly has a more complicated molecular formula than SiO_2, say $n$$SiO_2$, otherwise it would be a gas, analogous to CO_2, for $SiCl_4$ boils at 57° and CCl_4 at 76°. But would it be reasonable to say that the SiO_2 molecules are held together by the fifth valency of Si?

The only satisfactory answer to the above question is that in molecules R_2Cl_6 the two RCl_3 groups are held together by their own—let us say residual—affinity.

I think that the study of the question touched in the above lines will greatly add to the development of our theoretical ideas in chemistry, and deserves the careful attention of chemists.

B. BRAUNER.

Bohemian University, Prague, January 8.

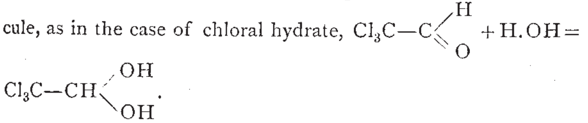
IN my letter, referred to by Dr. Brauner, I considered only the molecular formulæ of the chlorides, as indicated by the experimental results.

Dr. Brauner has now, in his interesting letter, enlarged the field of discussion by introducing the question of the constitutional formulæ of these compounds, involving the vexed question of the valency of the elements. Dr. Brauner points out the importance of the proof of the trivalency of these metals in their chlorides when in the gaseous state at high temperatures, and he then discusses the valency of the metals when their chlorides have the formula R_2Cl_6.

Although I am inclined to believe that the views expressed by Dr. Brauner may eventually prove to be correct, yet I cannot help thinking that we are not yet in a position to speak with certainty on one or two points. Dr. Brauner states that the number of "bonds" by which the atoms of the metal in the compounds R_2Cl_6 are united is certainly not *one*, and again, that Sn_2Cl_4 cannot be expressed by the formula Cl_2Sn'''—$Sn'''Cl_2$. But is this quite certain? Indium, the metal next to tin in the seventh horizontal series, is mono-, di-, and tri-valent in the compounds InCl, $InCl_2$, $InCl_3$; is it, then, quite unwarrantable to assume that tin is di-, tri-, and tetra-valent in the compounds $SnCl_2$, Sn_2Cl_4, $SnCl_4$?

As regards silica, $n$$SiO_2$, the properties of the compound seem to indicate that n is a large number, and here, unless we arrange the Si atoms in a ring, like the carbon atoms in benzene, we must probably look on the compound as "molecular."

It is difficult to find experimental evidence which bears directly on the question; perhaps such results as those obtained by Dr. Ramsay and myself on the dynamical and statical methods of measuring vapour-pressures may throw some light on the matter. We find that the two methods give identical results with ammonium chloride, nitrogen peroxide, and acetic acid, just as they do with all stable solids and liquids, but very different results with all the other dissociating substances examined. This appears to indicate some difference in the molecular arrangement of the two groups of compounds, but it is quite uncertain whether the difference is one between "molecular" and "atomic" compounds, or between compounds formed in a very simple manner, such as $2O_2N = O_2N{-}NO_2$, and those in which there is a breaking down of a stable molecule, as in the case of chloral hydrate,



$$Cl_3C{-}C\genfrac{}{}{0pt}{}{\diagup H}{\diagdown O} + H.OH = Cl_3C{-}CH\genfrac{}{}{0pt}{}{\diagup OH}{\diagdown OH}.$$

I am at present engaged in a study of the vapour pressures of halogen compounds by both methods, and it is just possible that some further light may be thrown on this question by the results of the investigation.

SYDNEY YOUNG.

University College, Bristol, January 15.

Remarkable Rime and Mist.

YOU have a letter in NATURE of January 17 (p. 270), signed "Annie Ley," which induces me to add the following:—We had here on January 6 an extraordinary rime formed at a temperature varying between 21°·5 and 25°·7 (1° warmer on the grass than at 4 feet), the air being almost calm. This rime increased in thickness and in length with the height above the ground. The length measured of the deposit on a birch-tree at 5 feet was $\frac{5}{8}$ of an inch; at 10 feet, 1 inch; at 15 feet, $1\frac{1}{4}$ inch; and at 25 feet, $1\frac{1}{2}$ inch. The hoar was nearly horizontal, pointing downwards at an angle of 15°. That deposited on the grass, however, was perpendicular, rising with a thin stem and having a large funnel-shaped head. Suddenly, at 10 a.m. of the 7th (next morning), the whole of the rime (still frozen) fell to the ground, and under the birch-tree of 30 feet in height and 18 feet across (sparse of branches, and none for 10 feet), the fallen rime covered the ground to the depth of rather more than 2 inches, and this, when melted, yielded 0·550 of an inch of water (or $3\frac{3}{4}$ inches of rime to 1 of water). The rime on the grass when carefully collected and melted yielded 0·033 of an inch. There was a dense mist whilst the rime was being deposited.

From this elevation (530 feet) we frequently look over dense mists that cover the water of the Bristol Channel and see the hills of Somerset, Devon, Monmouth, and Glamorgan. On the 19th instant, with a hoar frost, over the Bristol Channel was a dense dark mist apparently extending 100 feet into the air. This mist, at 8.30 a.m., rapidly changed on its upper surface to cirri clouds, and then to transparent vapour, and in an hour the whole mist by this process had disappeared. Elsewhere the sky was cloudless. These mists of the Bristol Channel change on their upper surface sometimes to cirro-strati, sometimes to cumuli, and twice they have been known to change to thunder-clouds during the last two years. The change to cirri has only been seen once.

E. J. LOWE.

Shirenewton Hall, near Chepstow, January 22.

Cercyonis alope and *nephele*.

IN his review (NATURE, December 27, 1888, p. 193) of my work on butterflies now in course of publication (in which I receive a far more generous treatment than I am accustomed to), Captin Elwes thinks me illogical in holding to the probable specific distinction of *Cercyonis alope* and *nephele*, and at the same time the specific unity of the Eastern American forms of *Cyaniris pseudargiolus*, *lucia*, *violacea*, and *neglecta*; and suggests as to the former that climatic differences in the regions they respectively occupy may have brought about the distinctions noted.

On general grounds, it seems in the highest degree probable that climatic differences have had much to do with the origin of the different forms in both cases, be they species formed or forming. But surely Captain Elwes is confusing the judgment when he fails to make a distinction between the successive seasonal forms of a digoneutic butterfly, as in the case of *Cyaniris* and de Nicéville's Indian species to which he alludes, and the synchronous variation of a monogoneutic species, like those (or that) of *Cercyonis*.

SAMUEL H. SCUDDER.

Cambridge, U.S.A., January 10.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

XIII.

Possible Accounts of the Faraday and Hall Effects.

THE account I have given of the magnetic rotation of the plane of polarization has made it depend on the phenomenon of hysterēsis, in a way which may be thus summarized. The value of μ for increasing magnetization is different from that for decreasing magnetization; an electric displacement such as occurs in every half-swing of a light-vibration is resolvable into two opposite circular components, one of which increases, while the other decreases, any magnetization already existing in the direction of the ray: the value of μ affects the speed of transmission of light; hence the two circular components will not proceed at the same pace, and the direction of vibration will infinitesimally rotate. The same thing is repeated at every half-swing, the elemental rotations being all in the same sense, and so the ultimate rotation of the plane of polarization in transparent bodies is accounted for.

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[1] Continued from p. 13.

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